

ANALYSIS

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DIRECTIONAL ANALYSIS AND BASIC FACTS

By L. SUSAN STEBBING

IT is not my intention to defend the account I gave of different kinds of analysis in the two papers of mine upon which Mr. Eugene D. Bronstein has commented in the last issue of *Analysis*. Those two papers are full of confusions and contain some very bad blunders, especially the earlier one entitled 'The Method of Analysis in Metaphysics.' But Mr. Bronstein has not dealt with these blunders, nor have I space to correct them here. I think that Mr. Bronstein's criticisms are due, to a considerable extent, to a misapprehension as to what it was I was trying to say. No doubt this misapprehension is due to the unclearness of my exposition. Accordingly, I shall try to restate some of the points referred to by Mr. Bronstein in the hope that, by so doing, I may reply to his criticism.

Mr. Bronstein's second and third points¹ are clearly the result of a failure to understand what I was trying to do. He has attributed to me views which I do not hold, and which I was expressly concerned to reject. I shall, therefore, take them first. Mr. Bronstein thinks that I have used directional analysis 'in order to justify the belief in basic facts.' On the contrary, I maintained that the method of analysis *as practised by certain philosophers* required the assumption that the analysis would terminate in basic facts, and that this assumption was not

¹To save space I am not repeating Mr. Bronstein's statements, but shall take them as read.

'certainly justified' and was not 'even very plausible.'² I stressed the point that it was an *assumption* that directional analysis is possible, and I tried (with very imperfect success) to set out what followed *provided that* the assumption was granted. This being so, I cannot understand why Mr. Bronstein says that 'directional analysis becomes the *deus ex machina*, introduced to save basic facts.'³

What I have just said has a bearing upon Mr. Bronstein's third point. It was never part of my intention to attempt to 'prove' that basic facts 'exist.' I cannot understand Mr. Bronstein's statement that 'it is nothing but a risky inference from a directional analysis to basic facts.' How could one speak of *inferring* basic facts from a method of analysis? His second point, therefore, does not seem to me to make sense. In other papers, to which Mr. Bronstein does not refer, I have twice urged that Bertrand Russell never seems to be clear as to whether what he calls 'the problem of the external world' is the problem of justifying a risky inference or is a problem of analysis. I hold that it is the latter. Consequently, I cannot see what criticism Mr. Bronstein is concerned to make.

Mr. Bronstein's main difficulty is that he cannot see any basis for the distinction between directional analysis and the analytic definition of a symbolic expression. At the outset Mr. Bronstein has been misled because he has not understood a statement he quotes from my lecture on *Logical Positivism and Analysis*. The statement is: 'Under the analytic definition of symbolic expressions I have time to consider only the analysis of complete sentences.' He takes me to be here contrasting sentences consisting of *non-verbal symbols* with *symbols* in the narrower sense, whereas the contrast intended is between *sentences* (however composed) and *phrases*. I quite agree that it does not matter 'whether we talk about the theory of descriptions in symbols or in English sentences.'

The fundamental difference between the analytic definition of a symbolic expression (whether an English sentence or an expression containing non-verbal symbols) and directional analysis is that the former is *analysis at the same level*, whereas the latter is not. The analytic definition of "A and B are first

² *Proc. Arist. Soc.* 1932-33, p. 92.

³ *Analysis*, p. 13.

cousins" would be "X and Y begat A, and W and Z begat B, and either X is a brother or sister of W or of Z, or Y is a brother or sister of W or of Z." This might be expressed more shortly by "One parent of A is a brother or a sister of one parent of B." An ordinary scientific definition of "rent," or of "conceit" would afford other examples of analysis at the same level. If the analysandum mentions individuals, so would the analytic definition; if apparent variables occur in the former they would also occur in the latter. For example, "The thing which has ϕ has ψ " can be analytically defined by "Something has both ϕ and ψ and it is not the case that something else has ϕ ". These examples are of two different kinds which, for certain purposes, should be further distinguished, but I have not space to do so here. This point, however, is not specially relevant since all the above examples are examples of analysis at the same level, i.e. are analytic definitions of symbolic expressions. But I wish to add one more example, since my use of it in the paper on 'The Method of Analysis in Metaphysics' may well have misled Mr. Bronstein. "Every economist is fallible" may be analytically defined by "Something is both an economist and fallible and it is not the case that something is an economist and not fallible." In the paper mentioned above I made use of the statement *Every economist is fallible* when I was trying to explain my technical usage of "referring to" and "indicating." My discussion was mainly concerned with directional analysis; it was accordingly a grave mistake not to make clear that I was *not* giving a directional analysis of *Every economist is fallible*. Nor, indeed, did I give an analytic definition of it. I still think that the point I was concerned to make about *referring to* and *indicating* was correct. But later in the paper I suggested that *Every economist is fallible* contains a configured element. I now think this is a mistake. It does not *contain* a configured element although it does *refer* to a configured element. I do not know whether it is this mistake which has misled Mr. Bronstein. He does not comment upon it, and the discussion in *Logical Positivism and Analysis*, from which he quotes, is free from this mistake.

Directional analysis is analysis at *other levels*. If we analyse a statement about a Committee into a statement about individuals, then the analysis is directional, and the levels are different. If we again analyse the statement about individuals into statements

about bodily and mental states, then the analysis is directional. The direction is *fixed*, in the sense that the direction is *from* statements about the committee *through* statements about individuals *to* statements about bodily and mental states. The direction is sometimes said to be from the less to the more ultimate. I prefer to say that the direction is from higher level non-basic facts to lower level non-basic facts, and finally to basic facts. This is the only sense of 'basic fact' which is required to give significance to the method of directional analysis. I agree that a philosopher might employ directional analysis without being successful in carrying the analysis to completion. That there should be differences of level is significant.

I do not think that anything I have previously said contradicts this last statement. That Mr. Bronstein thinks otherwise is, I think, due to the fact that he has failed to notice that I was concerned to discover the method which has been employed by certain philosophers who did wish to maintain that their analysis ended in basic facts (although they did not employ this terminology). I have avoided the phrase "atomic facts" because I think it has been used in conflicting senses, and I wished to avoid the associations due to these various usages. Space does not permit me to develop these points as I should wish. I must comment upon two other points raised by Mr. Bronstein.

The first concerns logical appropriateness. I do not see the point of Mr. Bronstein's difficulty here. I can only say that I introduced the phrase "logically appropriate" for the purpose of shewing how ordinary language may mislead us with regard to *logical* form. I was not there concerned with metaphysics.

The second point concerns the Note with which Mr. Bronstein ends. The basic facts upon which 'Hens lay eggs' is based are what they are notwithstanding any differences between the views of an idealist and a scientist. I fail to see how disagreement between an idealist and a scientist (if they do disagree) can in any way affect the question which direction leads to the desired goal. Surely it is possible to set off in a direction which leads to a *cul de sac*.

London, November 1934

EMPIRICAL FOUNDATION FOR LOGIC

By KARL BRITTON

IT has recently become quite common for people to say:

- (1) All a priori truths are analytically necessary: i.e. deducible from definitions.
- (2) All definitions are statement of conventions; they cannot be true or false. To ask "Why this definition?" is to raise a philological question.

It is clear that these statements are about definitions regarded as regulations for the proper use of words. Such regulations may be kept or broken. If broken, "improper" language, or even "nonsense," will be the result. But the definition is not falsified, nor can it be verified. In this it is rather like a *command*. Of course, if I give a definition and *add* the remark that this is how the word defined *is* as a matter of fact used by people, I shall be adding an empirical proposition, which clearly may be true or not.

The thesis of the statements above, is that all a priori truths are really of the same sort as the definition: their necessity just *is* their regulative character. It seems to me useful to make the following comments: many of the rules of a language used to express empirical propositions are themselves "founded upon" exact empirical propositions. This surely is a regular process in scientific writing. A proposition of the form:

In all observed instances, $r = f(A, B)$ (1)

(where r , A and B are measurable quantities) is an exact empirical generalisation. It *may be true*: it is widely held to *be true*. The structure of the language is then altered by the acceptance (usually for limited purposes only) of the definition:

$r' = f(A, B)$ Df. (2)

(Here " r' " denotes *via* " A " and " B ," whose denotation is unchanged. Unfortunately, we are apt to use the sign " r " for the new meaning as well as for the old.) For the "certain purposes," the sign " r " should no longer be used. It is a familiar fact that the definition looks very much like the empirical proposition. But of course (2) is a rule of language; it cannot be true or false; it is about the use of signs. And (1) is an empirical proposition, and is about r and A and B , not about the signs " r " or " r' " or " $f(A, B)$ ".

Note the close historical association between (1) and (2). We *can* most usefully ask : " Why do we use ' r ' and ' $f(A, B)$ ' as mutually substitutable ? " Of course the answer can't be in terms of " r " : we can't reply : " Because ' r ' means ' $f(A, B)$ ' ". Part at any rate of the answer will be in terms of " r ", a sign that has not ordinarily been used in the same universe of discourse as " r '."

The question " Why the definition (2) ? " may well be a philological one. But certainly it will also be asked by people interested in A and B and r , as well as by people interested in the history of " A " and " B " and " r ."

The definitions of empirical science are constantly being revised: why is that? Because the "facts" demand it. What are these "facts"? They are propositions about observations, expressed in an earlier language; a language that does not include the definition to be revised. E.g. In an engineering calculation, we have three forces acting at a point in equilibrium. We know the strength of two of the forces, and the directions in which they are acting. We *deduce* the strength and the direction of the resultant, R , by a definition of Statics. But the sign " R " at once suggests " R ", which is simply " the third of the three forces acting at a point in equilibrium. " This can itself be measured, and we shall have :

$R = n$ units, by experiment (e.g. strength of force)

$R' = f(P + Q)$, by definition.

Will R and R' be the same? If they are *not*, this *suggests* that the definition is a bad one. This definition was made only because it was found that :

In all observed cases $R = f(P + Q)$

Anything, therefore, that casts doubt on this empirical proposition, raises a *prima facie* objection to the definition founded upon it. But the answer to the question : " Why the definition (2) ? " must also (I think) include some statement as to the purpose towards which this definition is to be used. In the above case, for example, we are interested in the measurement of forces in equilibrium : it is this *purpose*, together with our beliefs about forces, that determine what definitions we shall use in statics.

So far I have been discussing only definitions to be used in

empirical sciences ; but many people are now content to regard such a statement as " Every proposition is either true or false, not both " (3) as a definition, in a broad sense of the word (See a note at end.) On this view, (3) is definitive of " proposition," no less than of " true," " false," and of " or." Now many alleged " unanswerable " or " meaningless " questions arise, because people ask :

Why do we use this definition or set of rules for words ?

Why not revise it ?

Are there no alternative rules ?

Such questioners *want*, I think, to be told something about the character of observed facts. Such an expectation would, as I have tried to show, often be quite reasonable in the case of a definition used in an empirical science : *is it* reasonable in the case of the definitions of logic or mathematics ?

Scientists now realise that any necessity that attaches to any of their propositions is simply the necessity of regulative definitions. They no longer claim that the empirical generalisations, upon which the definitions are founded, are themselves necessary, in any sense. Philosophers are sometimes inclined to say that the a priori propositions of logic are necessary in the same way that any definition is necessary. The question naturally arises : Are such a priori propositions founded upon empirical generalisations about matters of fact ? If not, have they any other " foundation " ?

We are raising the question : " Why the definition (3) ? " It seems to me that we can at least say something about *purpose*. For by " every proposition," we here mean only " every *empirical* proposition." Now the empirical proposition is a means for achieving a certain aim : not persuasion, not self-expression, but the conveyance of exact information about denotable objects. We want to assert or deny of such objects, some abstract notion. (The redness of *this* ; the urbanity of *him*.) And I think that the nature of this purpose does partly explain why we use the definition (3).

For this purpose is a *limited* one. Even the purpose " to say everything," is only one among many human purposes—and, moreover, one among many purposes for which *language* is an instrument. But the rules and definitions of logic aim primarily at producing an instrument for the " informative " use of lan-

guage. A definition, for example, is expressed in language, but it is not itself an empirical proposition. We can, of course, say that a definition is "sense or nonsense," and we *can* regard this as an interpretation of the rule " $p.v.\sim p$," as it applies to definitions. But this is really *quite* different from "true or false," the interpretation suitable to arguments that are empirical propositions.

And I believe that a great many kinds of "nonsense" (including some contradictions) are important human enterprises for which language is successfully (and therefore properly) used. If this is so, the term "nonsense" is an unfortunate one :

(i) It suggests that "nonsense" has been "shown up." As a matter of fact it has not been examined ; it is clearly a "blanket" term.

(ii) "Nonsense" might properly be used for *useless* jumbles of words. But everybody admits that many sentences are useful, even though they do not mean empirical propositions.

E.g. (1) "We are such stuff as dreams are made on." This conveys no information : it does not denote any parts or specific characters in our experience.

(2) "The Lord is thy Keeper." I who believe need no proof of this, and will accept no disproof. It expresses my feelings, and is of far more importance for me than many empirical propositions, e.g. : "A *really* looks after you," "R.M. is Prime Minister." Cf. Aristotle on the Orphic Mysteries : "The initiated are not supposed to learn anything, but to be affected in a certain way, and to be put into a certain frame of mind." (Quoted in Cornford's "Religion to Philosophy.")

(3) "All things change, and yet nothing changes." This is a contradiction, and is therefore nonsense from the point of view of the logic of empirical propositions : but it is successfully used to draw our attention to an important contrast of mood. And this it does by using the *same* word in association with both shades of feeling.

(4) The rules of logic hold only for perfectly defined terms : where terms are vague, there can be no certification a posteriori ; but how important for literature are "Types of Ambiguity," the work of Empson has demonstrated.

I feel quite sure that the rules of logic reflect only one of the purposes for which language is used. But are these rules further limited and conditioned ? Are there, as in the case of

statics, facts about reality, that make definition (3) a good definition? Is (3) founded upon empirical generalisations about the whole field of "topics for information?" Now *anything* may be a topic for an empirical proposition; we can say anything we like, about anything. This seems to involve that there are no propositions that are true of the whole field of empirical propositions and not beyond it (we should have to make type-distinctions, perhaps). That is to say, we cannot find any conditions within the field of logic, that would further limit the definitions of logic. This, I suppose, would be a common view of the question.

I suggest that something more might perhaps be said. For the conveyance of information, we use: (1) signs denoting particulars; (2) signs denoting abstractions. It might be said that it is a true empirical proposition about *all abstractions*, that they may be realised in the concrete particulars, or may be excluded. Upon this empirical proposition, is founded the rule: $p.v.\sim p$. If it were objected that the alleged empirical proposition must surely be part of the definition of Abstraction, it might be replied: Then it is an empirical proposition, expressing a (contingent) fact, that there is any abstraction at all. Abstraction is a psychological peculiarity of certain organisms. The fact is contingent in just the same way that it is contingent that there are mammals or men or meteorites. On the other hand, metaphysicians have claimed that Abstraction is much more than this.

Whitehead: "Symbolism," p. 30: "Abstraction expresses Nature's mode of interaction and is not merely mental. When it abstracts, thought is merely conforming to Nature—or rather, it is exhibiting itself as an element in Nature."

Whitehead: "Science and the Modern World," p. 228 (Cap. X "Abstraction.") "It is the foundation of the metaphysical position which I am maintaining that the understanding of actuality requires a reference to ideality. The two realms are intrinsically inherent in the total metaphysical Situation."

Cf. also, The "Parmenides," 132 b, c.

These metaphysicians claim that " $p.v.\sim p$ " is based upon a *true-or-false* proposition, which, in some way, is *necessarily* true.

NOTE: I have used "definition" in a very broad sense. The rule $p.v.\sim p$ is either a member of, or a derivative from, a set of

postulates (or "primitive propositions") which regulate the use of the logical constants and of "p" (such a set is to be found, of course, in "*Principia Mathematica*", 1st edition, Vol. I, p. 94). I shall say that the whole set defines all the elements. That is, it regulates the use of all the signs in it. Just as 1.01 *defines* the use of the "horse-shoe" by reference to " \sim " and " \vee "; so *all* the postulates or assumptions *define* the use of all the logical constants and "p" by reference to each other.

Aberystwyth, November 1934.

REPORT OF LECTURES ON *PHILOSOPHY AND LOGICAL SYNTAX*, DELIVERED ON 8 10 AND 12 OCTOBER AT BEDFORD COLLEGE IN THE UNIVERSITY OF LONDON, BY PROFESSOR RUDOLF CARNAP

C. A. M. MAUND AND J. W. REEVES

PROF. CARNAP explained that he sought to show a new method of philosophizing. The problems dealt with in traditional philosophy are of various kinds, three of which may be distinguished as Metaphysics, Psychology, and Logic. These should be regarded as three components, since most theses and questions combine a metaphysical, a psychological, and a logical component. The first of these must be rejected. Only the propositions of mathematics and empirical sciences have sense. We often suppose that propositions which have only an expressive function also assert something. Nearly all movements and words express something but many assert nothing and are neither true nor false. Linguistic utterances which can be verified are representative and have theoretical sense. A proposition P, which may be individual or general, can be verified either directly, by present perception, or indirectly, by direct verification of propositions deduced from P and other already verified propositions. If P is not directly verifiable, it is an *hypothesis*; as such it cannot be certain since the number of propositions necessary for its complete verification is infinite. To say that P is an hypothesis is to say something about its logical character;

it is not to say that the probability of its truth is low. No statement which is not in principle directly or indirectly verifiable has sense. It follows that all propositions of metaphysics, that is, all propositions which pretend to represent something over and beyond experience, all doctrines such as Realism, Idealism, etc., usually called epistemological and taken to assert or to deny the reality of something, all propositions of normative ethics, are unverifiable; accordingly, they have no sense. Empirical propositions asserting reality have sense, since they assert something about part of a system, e.g. that things of such and such a sort are to be found in the world, they do not assert something about the system as a whole. Psychology and ethics are empirical sciences about the actions of human beings. There remains, therefore, logical analysis as the proper task of philosophy. In order to be consistent it must be shown that the propositions of logical analysis are themselves neither metaphysical nor psychological.

The logical syntax of a language is the formal theory of that language; a formal consideration has no reference to meaning or sense. Such an investigation is not as narrow as it appears to be, since many questions expressed in the non-formal mode can be translated into the formal mode, e.g. Hilbert's metamatematics. The formal method of Hilbert is applicable not only to mathematics but also to the whole language of science, or to any other language. A language is a system of rules of speaking as distinct from acts of speaking. There are two kinds of rules: (1) *Formation Rules*: these concern the method by which sentences may be constructed out of different kinds of symbols. They determine that a series of words is a sentence in a given language-system *S*, only if it has such and such a form. The formation rules of natural languages are too complicated to be given completely. (2) *Transformation Rules*: these determine how from certain sentences others may be derived. The totality of the formation rules of *S* is the definition of 'sentence in *S*.' The totality of the transformation rules of *S* is the definition of 'direct consequence in *S*.' 'Sentence' and 'direct consequence' are the chief primitive terms of logical syntax. All definitions are in terms of these, and are all relative to a given language-system. Only the most important can be given here.

A sentence of a system *S* is *valid*, if it is a consequence of the

null-class of premisses ; *contravalid*, if every sentence is a consequence of that sentence ; *determinate*, if it is either valid or contravalid ; *indeterminate*, if it is neither valid nor contravalid.

A system may contain both logical and extra-logical rules. Thus we must distinguish between two kinds of rules. *L-rules* are transformation rules of a purely logical or mathematical character. *P-rules* have an extra-logical or extra-mathematical character. A sentence is an *L-consequence* of the premisses, if it is connected with them by a chain of sentences constructed according to *L-Rules* ; it is a *P-consequence* if the chain is constructed according to *P-rules*.

A sentence is *analytic* if it is an *L-consequence* of the null-class of premisses, i.e. true by *L-rules* alone ; *contradictory*, or *L-contravalid*, if false by *L-rules* alone ; *L-determinate* if it is either analytical or contradictory ; *synthetic*, or *L-indeterminate*, if *L-rules* do not suffice to determine its falsehood.

A sentence is *P-valid*, if valid but not analytical ; *P-contravalid* if contravalid but not contradictory. The *content* of a sentence is the class of non-valid consequences of a given sentence. Two sentences are *equipollent* if they are consequences of each other, i.e. have the same content. Two expressions are *synonymous* if the content of any sentence containing one of them is not changed by the substitution of one expression for the other. A *syntactical quality* q^2 is *parallel* to a quality q^1 if the designation of a possesses q^2 when, and only when, a possesses q^1 .

Two questions may be raised concerning the sense of a set of symbols. (1) Has this series of words sense, where 'sense' signifies theoretical sense ? This question may be answered by reference to the definition of sentence in that language-system. (2) *What* sense has this sentence ? This may be answered by reference to content. Even after the elimination of metaphysics, philosophical sentences seem to concern something besides syntax. This appearance is deceptive. We have to find the conditions under which sentences have this deceptive form. To explain this deceptive appearance sentences must be divided into three kinds : (1) syntactical, e.g. 'The word "rose" is a thing-word' ; (2) real-object-sentences, e.g. 'The rose is red' ; (3) pseudo-object-sentences, e.g. 'The rose is a thing,' which are like syntactical sentences in form but like real-object-sentences in content. A sentence is a pseudo-object-sentence if it attributes

a quality to an object such that for this quality we can find a parallel syntactical quality. Classes (2) and (3) belong to the material mode of speech, (1) to the formal mode. It is with syntactical and pseudo-object-sentences that logical analysis has to deal. Real-object-sentences fall within the domain of empirical science. Most philosophical sentences are deceptive because they are formulated in the material mode. Many philosophical problems are due to the fact that the material mode conceals an incompleteness of thesis, whereas in the formal mode the reference to a particular language makes the problem clear. For example, the dispute whether numbers are classes of classes or primitive objects arises from a failure to see that all philosophical theses are relative to a language-system. Sentences in the material mode lead us to ask pseudo-questions about the real essence of things, about the nature of space, time, causality, about the nature of modalities, and so on. But, for example, 'x is impossible' is a veiled syntactical sentence capable of formulation in terms of 'contradictory' or 'P-contravalid.' All such problems become significant only when they are free from empirical elements and translated into the formal mode. For example, the two theses 'A thing is a complex of sense-data' and 'A thing is a complex of physical matter' only seem to be incompatible because they seem to concern the essence of things. Translation into the formal mode shows that each asserts the possibility of a certain transformation of thing-sentences; as both transformations are possible, both theses are true. Thus there is no special philosophy of nature but only a philosophy of science; there is no philosophy of life, of mind, of history, or of society, but only philosophies of biology, of psychology, of the historical and social sciences. In this statement 'philosophy' signifies syntactical analyses of the language-systems involved. Thus the philosophy of any particular science is always a syntactical analysis of the language of that science; the principal problems concern the character of the terms and of the sentences of the language, especially the transformation rules connecting it with other languages. Among the languages concerned, the language of physics is all-important. It is the basic language in the sense that any sentence in any scientific language is equipollent with one in the physical language. Thus for every psychological quality Q^1 there is a corresponding physical quality

Q². Doubts whether to every psychological quality there corresponds a physical quality can be met on the grounds that, unless psychical states have some physical expression which is observable, all statements concerning these states would be unverifiable and without sense. The argument that there is a psychological predicate originally used only to describe one's own mental state, recognised only by introspection, does not show that the predicate is not expressed ; in using the predicate in speaking or writing, we do express it. Moreover, even a predicate originally used only in regard to the speaker himself, and founded on his introspection, can be used by him, with regard to another, as a result of the linguistic expression of the other.

This thesis, named 'Physicalism' by Dr. O. Neurath, is often misunderstood because it is taken to assert what it does not assert. Physicalism does not assert that to every psychical state there corresponds a physical state of the body. Such a statement belongs to the material mode of speech. But it is easily confused with the correct physicalistic statement : 'To every psychological sentence, say S¹, there is a corresponding physical sentence, say S², so that S¹ and S² are equipollent on account of certain valid laws.' If this statement be formulated in the material mode of speech, pseudo-problems arise, and we slide back into metaphysics, for example, the discussion of the psycho-physical problem. Whether, when using the material mode, we should speak of a psychical or of a physical state, or about one only, merely concerns the use of language ; it is a matter of taste, not a question of fact.

The thesis of the unity of science is closely connected with Physicalism. This doctrine is equivalent to the view that the *terms* of all branches of science are logically uniform. The doctrine is misunderstood if it be taken to assert that the *objects* of every branch of science are of the same kind, and that assertion be taken as equivalent to the assertion of monism. That would be a metaphysical assertion, due to a careless use of the material mode of speech. But Physicalism is not a metaphysical, but a logical doctrine ; that is to say, Physicalism is a syntactical thesis.

This explanation of what Physicalism asserts, and the doctrine of the unity of science, have been introduced only to illus-

trate the view that all theses and questions of logical analysis, and all philosophical problems (in the sense in which 'philosophical' has been used in these lectures) belong to the method of logical syntax. That is to say, they should be dealt with by the analysis of the formal structure of language as a system of rules. This method of logical syntax is the method of philosophy.

In the course of various discussions held during Carnap's visit to Bedford College, he admitted a change in his point of view, or method of approach, since the writing of his book *Der Logische Aufbau der Welt*. Formerly, both he and Schlick had emphasized the importance, for their view, of Bertrand Russell's distinction between acquaintance and knowledge by description. Carnap would not now begin his investigation by seeking for *Elementarerlebnisse*. On the contrary, his present approach is entirely through considerations of language; hence the importance he now attaches to the distinction between the formal mode and the material mode of speech. Only the germ of this distinction is to be found in his earlier work. It is in virtue of this distinction that Carnap can insist that every significant question asked by philosophers is either one concerning logical syntax or is a question which falls into two parts, one belonging to some one or other of the particular sciences, the other to logical syntax. Thus, in replying to a question whether there was not a problem concerning the relation between 'lived time' and 'physical time' (or 'time' as it occurs in physics), Carnap replied that no problem was involved which did not fall, on the one hand, into the domain of psychology, on the other, into that of logical syntax. The problem cannot be solved until it is divided into these two parts, a psychological part, and a logical part. In logical syntax the fundamental term is 'direct consequence,' and the fundamental problem is the construction of language-systems.

Carnap admitted that there was a division of opinion among the Viennese group with regard to the question of atomic facts. He would not himself admit that there are atomic facts; Schlick on the contrary does admit them (see Schlick's article 'Ueber das Fundament der Erkenntnis,' in *Erkenntnis*, Bd. 4, Heft 2). Asked whether 'This is red' or 'This is a box' would be an example of a protocol-sentence, Carnap replied that either

sentence might be regarded as a protocol-sentence. The distinction between protocol and derivative sentences is to be determined only in terms of 'direct consequence in the language-system L.' Whether or not a language-system is applicable can be decided only by reference to propositions falling wholly within a particular science.

Bedford College, London, November 1934.

ERRATUM

Vol. 2 p. 31 l. 7 from bottom, for 'Mr. Cornforth replied. . .'
read 'Mrs. Cornforth replied. . .'

